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## ***Interactive comment on “Weather response to management of a large wind turbine array” by D. B. Barrie and D. B. Kirk-Davidoff***

**Anonymous Referee #2**

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This paper examines the transient response of the CAM model to changes in surface roughness. It shows that a large-scale change in surface roughness affects synoptic scale flow. The impacts affect downstream areas within a few days. This result is interesting but not entirely surprising given that Kirk-Davidoff and Keith [2008] found large equilibrium response to surface roughness anomalies in the same model. This paper would be nice supplement to Kirk-Davidoff and Keith [2008].

My biggest concern is about the way the problem was contextualized. If this was a paper on surface roughness, I would have been satisfied. But I am not convinced that a wind farm can be approximated as only a change in surface roughness. The authors mention that Baidya Roy [2004] adopted a different approach. I would like to see more discussions on why the authors chose this approach. Are there any field data to back

this up? Will the effect be same for momentum, heat and moisture roughness lengths? Does this take into account that a wind farm, unlike a mountain, is a porous obstacle?

I also found the title a bit misleading. I was expecting to see something like how different configurations of wind farms affect weather. There is nothing in the paper about "management" of wind farms. Also, the introduction discusses the literature on "intentional modification of weather" while the topic is more like "inadvertent modification of weather". I did not get a sense that authors are suggesting that wind turbines be used to modify weather like cloud seeding and other experiments.

A quick literature review shows that there are 3 papers on wind energy and climate. The authors cite 2 of them but Keith et al [2004, PNAS] is not cited at all. This is a glaring omission.

I buy the authors argument that wind energy is going to be big in the near future. Hence, this is a very timely topic and should be addressed urgently. The authors have the expertise to do so. I hope that they will continue exploring this topic, address all the issues raised above and resubmit this paper. However, I suggest that they present their work as a theoretical problem with real-world applications (like Kirk-Davidoff and Keith) instead of a purely applied problem. If they choose the latter track, then they must ensure that their assumptions are realistic, e.g., a wind farm must look like a plausible wind farm from the near future.

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